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Interactive comment

Interactive comment on "On the Resonance Hypothesis of Tsunami and Storm Surge Runup" *by* Nazmi Postacioglu et al.

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article

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Discussion paper

Responses to the referee-I

1. The referee refers to "Bowers,JFM and Mei (1983)" and says that when the channel depth is constant and equal to that of the outer sea, the linear and nonlinear resonance mechanics can be found in these texts.

   We disagree that Bowers (JFM) is relevant to our discussion because the resonance phenomenon in this article originates from a completely different mechanism in the sense that in Bower's work a larger semi-infinite channel connects to a finite-length narrow bay. In his case, the waves scattered towards offshore from the channel mouth bounce back from the sides of his finite-width wider channel. In our case the bay opens to a semi-infinite ocean and the scattered waves freely progress offshore. His problem is actually more complicated than ours, that is why he did some simplifying assumptions such as averaging over the width of the wider channel which is not relevant in our case. However, one of the referees in the JFM submission directed us to another work from seventies, Kaijura 1977 (K. Kajiura. Local behaviour of tsunamis. In D. Provis and R. Radok, editors, Waves on Water of Variable Depth, volume 64 of Lecture Notes in Physics, pages 72–79. Springer Berlin / Heidelberg, 1977.) which is very relevant to our case.

   We did use Mei's book for his conformal transform approach. However this technique is limited to long-wavelength (wavelength much larger than the channel width) and thus not applicable to find the transient response which involves *both* high and low frequencies. So we resort to an integral equation approach and the compare the results with those obtained using conformal mapping (Figure 13 in the original submission).

2. The referee argues "The linearized theory is used. For both Models matching at the shelf break is done by linearizing the Carrier-Greenspan solution in an ad hoc manner without checking whether nonlinearity is locally important."

   In the revised version we dedicate a new section to this issue (we also added an appendix). In both the original and revised manuscripts we have assumed a monochromatic small-amplitude incident wave. Near the resonant frequencies the amplitude of the standing wave over the slope is much larger than the amplitude of the incident wave. Therefore, in the revised version, using a perturbative approach, nonlinear effects are taken into account even in the deeper part of the channel. The boundary condition at the toe of the slope are accordingly perturbed. We limited ourselves to the first-order perturbation, so the frequencies are not affected. The important conclusion here is that the effect of nonlinearity arising from the boundary condition at the toe is 3 or 4 times smaller than the nonlinearity arising from shoaling. Thus, as far as the runup is concerned, the nonlinearity due to the boundary condition at the toe is not critical.

3. The referee mentions "Throughout the paper the authors made extensive reference to past works on tsunami without considering the difference from their own work of persistent and periodic forcing . Tsunami is strictly transient where a finite number of leading crests are the most important to runup. In contrast resonance by periodic forcing takes a long time to reach quasi steady state which is important only at the end rather than the beginning. Hence the frequent and elaborate citations are mere digressions which only interupts their line of reasoning. ". He/she adds "Here are a few examples (1) The long paragraph on p 6 starting with "Recently Stephanakis..." (2) The paragraph on p 8 " In an effort to calculate solitary wave runup...), and (3) The paragraph on p 9 starting with "

The geometry considered by Stephanakis...". And many more. They can all be shortened or eliminated."

The referee seems to be particularly insistent on persuading us not to cite Stephanakis' papers. We are not inclined to do so to keep the integrity of the manuscript which dwells very much on the discussions on the few recent works on runup resonance. Despite the addition of a new section and appendix, we did substantially shorten the manuscript (it is now 29 pages instead of 36 pages).

It is indeed true that a Tsunami is a transient but we do calculate the time to reach the steady-state and therefore can pinpoint strict time limits for the transient duration. We believe this is important (see Figure 12 in the original manuscript).

4. The referee mentions "or a physically straightforward topic the mathematical treatment here is very convoluted and can be more systematically presented. "

   We agree with this criticism and made an effort to shorten the mathematical derivations while, at the same time, keeping the manuscript's main important ideas.

5. The referee mentions "The authors never question whether in this nonlinear system chaos can be induced by simple harmonic incident waves."

   Chaos is a whole different topic which is not within the scope of the present manuscript. Furthermore, to chaos to ensue (if any), long durations are necessary which we do not consider.